LETTERS TO THE EDITOR.

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A Measure of the Intensity of Hereditary Transmission.

THE possessors of certain hereditary characters are unquestionably *sub-prolific;* that is, they eventually contribute less than their average share to the stock of the future population. It may be that they die before the age of marriage, or that they are sexually unattractive or unattracted, or that if married they are comparatively infertile, or that if married and fertile the children are too weakly to live and become parents. It is very probable, though I have no trustworthy facts to confirm the belief, that persons affected with hereditary insanity are subprolific because their families, if they have any, are apt to contain members who are afflicted in various ways that render them less likely than others to live and to marry. But I do not propose to go into the details of this or of any other malady, but merely mention it as an illustration of what is meant, when I assume that the possessors of some particular characteristic, not necessarily a morbid one, and which may be called A, are subprolific on the average.

It is a familiar statistical fact that the characteristics of a population, taken as a whole, who live under uniform conditions, change very little during many successive generations. So many per million of them are always found to be affected in this way, so many per million in that. The birth-rate continues the same, so does the death-rate; similarly as regards the various kinds of accident, and also, it may be inferred, as regards each form of disease, though it would be difficult to prove this in all cases, owing to improvements in diagnosis and nomenclature which make the statistics of disease for one period not comparable on strictly equal terms with those of another. It is therefore reasonable to discuss what might occur in an ideal population, which we will call P, whose characteristics are absolutely unchanged during successive generations, and to make such small corrections in the results as the conditions may require when dealing with real populations.

P and A being thus defined, it is obvious that the characteristic A must be transmitted with exceptional intensity in P. The possessors of A leave comparatively few descendants, consequently those few must be over-richly endowed with A; otherwise the number of the possessors of A would steadily diminish, and a P population would be impossible. Wherever a P population occurs, there must exist an inverse relation between the intensity with which A is hereditarily transmitted, and the prolific faculty of those who possess it.

This consideration may be of practical importance to actuaries in enabling them to estimate more justly than at present, the weights to be assigned to different hereditary diseases. It is a most difficult and delicate matter to attack this question directly, namely by making exhaustive inquiries into the life-history of all the near relatives of those who suffer from any serious hereditary malady. The difference in the results arrived at by different inquirers proves this, and shows the need of some second and independent method of investigation. The above considerations supply such a method in all cases where the frequency of the disease is found to have been approximately constant during successive generations of the population taken as a whole.

All that will then be needed, is to find how far those affected by the disease in question have been prolific, testing their capacity in that way by the number of their adult descendants in (say) the *second* generation, those in the first generation indicating little more than their fertility, which, as the children may be weakly, is not the same thing as the capacity of the parents for contributing to the future population. When the descendants in the second generation are neither more nor less numerous than the generality, the intensity of the transmission of the disease would be the same as that of any neutral quality, such as a moderate difference of stature. But if those descendants were more numerous than the generality, the intensity of transmission must be less than the average, while if the descendants were less numerous, the intensity would be greater.

It must be clearly understood that this method is of general application, and is not intended to be confined to morbid characters only. FRANCIS GALTON.

Triboluminescence

THE interesting list of substances mentioned in to-day's review in NATURE of a paper on the subject of the above phenomenon, mentioning as substances in which it is conspicuous, cane-sugar, saccharin, hippuric acid, and some still more complex organic bodies, might lead one to suppose that only substances of an organic nature, in a crystalline state exhibit the kind of triboluminescence seen as a flash of light when a crystal of such substances is crushed between two glasses. But this is not quite exclusively the case, because crystals of uranium-nitrate, and perhaps other crystallised salts of uranium, emit a very bright greenish-yellow flash when pressed to pieces between glass plates. The property seems permanent in these crystals, and it is also apparently independent in them of chemical impurities, since any crystallised sample of the nitrate, as far as I have tried, shows the light flash very strongly, without any apparent loss of brightness by long keeping.

The ruddy light which gleams from under glass or from a flint pebble when ground with strong pressure on a grindstone, must apparently be a true example of luminescence produced by friction, since it is equally visible under water on a thoroughly wet, as on a dry, grindstone, where it can hardly be supposed to result from high temperature producing actual incandescence. But examples of crystals which emit light by fracture do not, it appears, present themselves in nearly such abundance among mineral substances, as they have now been shown in the above-mentioned paper to do in so many cases among organic bodies.

A rather interesting observation of thermoluminescence once befel me while making trials of that property in minerals; and as it may afford, perhaps, a ready means of tracing lime or calcareous ingredients in certain minerals, it may be useful to mention it here, although the mode of excitation used in that instance was not by crushing or rubbing, but by heating the material. Some fine dust and grains obtained from the interior portion of the mass of the Middlesborough aërolite, when the meteorite was first being chemically and microscopically examined, were found, to my considerable surprise, to glow quite distinctly, though not very brightly, with yellowish-white light, when sprinkled in the usual way for these experiments on a piece of nearly red-heated iron in the dark. No such luminescence would, I believe, be evolved by that means from pure terrestrial specimens of the pair of double silicates of magnesia and iron (olivine and bronzite, much less from the moderate sprinkling of nickel iron, and perhaps of iron-sulphide found with them), of which the stony matter of the meteorite in the main consists. But as its stony mass was considered, in the exact chemical analysis of the meteorite made by Dr. Flight,[1] to contain probably, besides, an appreciable amount of labradorite or lime-felspar, the source of the light may have been this calcareous ingredient of the stone, as calciferous rocks and minerals, for the most part, shine brightly with various shades from light- to reddish-yellow, in the dark, when strongly heated. To whatever chemical materials in the stone, however, the light was really due, it afforded, at all events, clear proof that no heat of exceedingly high temperature can ever have penetrated to the interior of the meteorite, even when it was passing at its fall, in a fireball through the atmosphere, since the time when it was broken off from some parent rock and projected on a celestial course about the sun; for a very moderate degree of heat suffices to expel completely from minerals of these luminescent natures all the store of thermoluminescent energy which, either originally communicated to them from without by radiation near some exposed or denudated surface, or else contracted by them in some more mysterious way at great subterranean depths, they more or less abundantly possess. A. S. HERSCHEL.

Observatory House, Slough, April 27.

The New Zealand Godwit (*Limosa novæ-zelandiæ*).

THE Maori of New Zealand have an ancient saying or proverb, "Who can tell where the kuaka (the godwit) has its nest?" No doubt the Maori were well acquainted with the singular habit of these birds, in that they leave the shores of New Zealand, for a distant land across the seas, about the same time that other migratory birds, which have wintered on the Pacific Islands located nearer the tropics, are nesting and

[1] *Proceedings* of the Royal Society, vol. xxxiii., p. 347, February 1882.

rearing their young in the New Zealand forests, to which country they periodically return for the summer season. Such, for example, are the long-tailed cuckoo and the small bronze-cuckoo, known to the Maori as "the bird of Hawaiki"—that is, the bird who returns to the land from whence the Maori ancestors originally came.

Our kingfisher also moves northward in the autumn, and may likewise leave for a warmer country. These latter birds conduct their migrations as we should expect—that is, they reverse the conduct of their flight to those birds which live in northern latitudes, and we feel that their natural instincts are working according to rule. But the kuaka, not satisfied to pass the winter in a warmer country, must actually have two summers—one in New Zealand and a second in Northern Siberia, where it is said to have its breeding-place. Any way, it leaves in countless numbers from the north-east point of New Zealand, from almost the very place where the spirits of the dead Maori are supposed to take their departure to the other world (Reinga). For which reason the bay on the shore of which the birds assemble before flight is named by Europeans "Spirits' Bay."

The Polynesian mariner may in former times have guided his migrations by observation of the place of departure and arrival of birds of passage, also from the particular dates of such occurrence, and from the circumstance that the winds at that time were most favourable for travel in such particular directions. The spirits of their dead may have been supposed to return to the original birthplace of the race; and the nearest point of departure would be that from which the birds also departed.

But do any migratory birds other than the kuaka go further north than Tahiti, Rarotonga, Samoa, and the Fijis?

I always understood that no bird from either the north or the south temperate zones ever voluntarily crossed the tropics, and to me it seems a fable that even the kuaka should do so.

Whence comes the hereditary knowledge that should lead the kuaka half over the world to find a suitable breeding-place? Why does it not go in search of an Antarctic continent, as should be the natural sequence of events? Are not the high lands and alpine valleys of New Zealand where the dotterel, the red-breasted plover, the stilt-plover, oyster-catcher, &c., make their nests, equally suitable for the godwit?

Where does the European godwit (*Limosa lapponica*) breed? and is it not said that the nesting-place of the European knot (*Tringa canutus*) has never been discovered?

That the New Zealand godwit starts in a northerly direction in its migration is assured; but who has traced its course onward, as following the shores of China, it is making its way to lonely steppes in Siberia?

That these birds should winter during a New Zealand summer, and then leaving should pass through both temperate and torrid zones, and still onward to the confines of the north frigid zone to nest and summer, is truly marvellous. Will any reader of NATURE kindly contribute to our knowledge of the nesting-place of the godwit or the knot, or remark on other points at issue?

TAYLOR WHITE.

Wimbledon, Hawkes Bay, N.Z., February 9.

In reference to the above, the British Museum possesses a single egg of the knot, said to be one out of a clutch of four obtained at Disco Island, Greenland. Colonel Feilden has good grounds for believing that this bird nests in the New Siberian Islands.—ED.]

The Indian Musk-Shrew.

THE old yarn about the tainting of wine in bottle by the common Indian shrew (*Crocidura coerulea*)) seems to die hard, since "W. T. B." has had to correct it again in your issue of this week. The account of a crucial and deliberate experiment may be another nail in its coffin.

I kept wine in small chambers off my office, in a locked basket, ventilated at the ends, for use at luncheon. One day I opened it, and found a musk-shrew coiled up on a napkin, and did not disturb him, nor he himself. Next day I impanelled an unconscious jury; and we found the wine perfectly good. The musk-rat had been there in the morning, but had received a quiet hint to go. When my guests were gone, I wiped a glass with his napkin, filled it with wine from the same bottle, and found this too musky to swallow.

The wine was a sound Pommard from Treacher and Co., Bombay, with capsuled corks bearing their stamp.

I do not know whether it was bottled in Europe or in India.

I believe that the commonest cause of the musk-taint in wine is the wiping of the glass with a clout that has been picked up out of a corner, where the musk-shrew has laid on it.

Even in the best houses in India native servants will often use very little care about the cleanliness of "glass-cloths"; and when one that has served to clean a lamp or shelter a shrew is next used upon a wineglass, you have *vera et sufficiens causa* for spoilt wine—and temper.

I have a note on this somewhere in the *Journal* of the Bombay Natural History Society; but it is buried out of sight in some back volume, as my experiment took place about twenty years ago. I may add that the place of it was Ahmadabad, in Gujarat.

W. F. SINCLAIR.

102 Cheyne Walk, Chelsea, London, S.W., May 5.

Mammalian Longevity.

SINCE my letter on this subject in NATURE of March 23, I have noticed that a slight change in the formula—the reduction of the constant from 10·5 to 10·1—gives much better results. The agreement is now very close indeed. The amended statement now runs as follows:—

The full term of life in any mammalian species is equal to 10·1 times its period of maturity divided by the cube root of the period, or 10·1 times the cube root of the square of the period.

We get the following results from its application:—

Animal.	Observations.			f. t. l. by formula.	Other observations.
	Authority.	p. m.	f. t. l.		f. t. l.
Dom. Mouse ..	Dr. Ainslie Hollis.	·25 yr	4 yr.	4 (4·01)	
Guinea-pig ...	Flourens.	·583	6–7	7 (7·05)	
Lop-Rabbit—					
Buck... ...	R. E. Edwards.	·75	8	8 (8·3)	
Doe	R. E. Edwards.	·67	8	8 (7·7)	
Goat	Pegler.	1·25	12	12	
Fox	St. G. Mivart.	1·50	13–14	13·25	
Cat	Jennings.	2	15	16	
Cattle	Dr. Ainslie Hollis.	2	18	16	14, Gresswell. 15-20, Flourens.
Large Dogs ...	Dalziel.	2	15	16	15-20, Flourens and others.
Thor. Horse ...	Dr. Ainslie Hollis.	4·5	30	28	
Pigs	James Long.	5	30	30	
Hippopotamus	Chamb. Encyc.	5	30	30	
Lion	St. G. Mivart.	6	30–40	33	
Hunter... ...	Blaine.	6·25	33	34	
Arab Horse ...	Dr. Ainslie Hollis.	8	40	40	
Camel	Flourens.	8	40	40	
Man	Buffon.	25	90–100	86	100, Flourens. 75, Farr.
Elephant ...	Darwin.	30	100	98	
Elephant ...	C. F. Corder and Indian hunters.	35	120	108	100, Darwin.

In this table, p. m. stands, as before, for period of maturity, and f. t. l. for full term of life.

In the first table another statement dealing with the cat was also given, on the authority of Dr. Mivart, which is excluded from this, since the period mentioned—one year—obviously refers to the animal's period of puberty, not its period of maturity, as is indicated by Dr. Mivart's expression: "The domestic cat begins to be ready to reproduce by the end of the first year of her life. . . ."

The age of the hunter, calculated from Blaine, was given in the previous table at thirty-five, and in this it is given at thirty three. Blaine states that a horse of thirty years is relatively as old as a man of eighty, and a horse of thirty-five as a man of ninety. The first formula gave about ninety for man, and the corresponding age for the horse was therefore thirty-five; but the corrected formula gives eighty-six for man, which corresponds to thirty-three in the horse.

I agree with Dr. Ainslie Hollis that Buffon's 90-100 years for man is too long; but, on the other hand, seventy-five—the period given by Dr. Hollis from Dr. Farr's calculations—seems much too short. The great majority of persons have their lives cut short by disease, the nervous strain of life, &c., and do not live to anything like the full term of life. Were it not for such influences as these, most persons at seventy-five would probably still possess a considerable degree of vitality, and should be able to look forward to many years of life. Furthermore, Farr's cal-